Meteorological Method

BY

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IT is generally admitted that the development and progress of Meteorology are not at all in proportion to the labor and intelligence devoted to the study of its problems.

This fact has become more evident during the past five or six years, during which, the Meteorological institutions, established by the liberality of various governments, have been busy with observations, endeavoring to lay the ground-work of a science so important to general welfare. But while the material thus collected grows to gigantic proportions, no master-hand has been found to arrange it in its proper order, so that the laws that lie hid may be clearly seen. Indeed, already, serious apprehensions of entire failure are felt, and the English Parliament has charged a special committee with the duty of enquiring whether the government is justified in continuing its outlay upon an institution, the results of which are apparently so small. For the deduction of general laws has been, as yet, almost an entire failure, and the practical work of weather-predicting and storm-warning, not of very great utility. It is true, and has been gratefully acknowledged, that, in the majority of cases, the predictions of the English Meteorological Bureau have been verified; but these cases are, for the great part, the unimportant storms, and the most serious and destructive movements have come unheralded and unforeseen. In the presidential address of Dr. Thomas Andrews, before the British Association for the Advancement of Science, about a year ago, he says of the previous year's work of the Meteorological Office in London:—"Few storms occurred for which no warnings had been given, but unfortunately these were some of the heaviest gales of the period."

And also, in the nature of the case, a signal service can render little assistance in distant agricultural sections, or to ships out at sea, where its aid is most needed, except by the discovery of laws that shall enable the husbandman and the mariner to do their own predicting.

But the most serious aspect of the affair is this—that some of the scientific experts called into council by the English Parliamentary Committee, express the opinion that,—to quote the words of Airy,—"Meteorology is no science." Now, although this condemnation is certainly too sweeping and too severe, it is nevertheless partially justified, and it should induce investigators and directors of meteorological institutions, to profit by and acknowledge the results of the labor of others, however much these results may militate against their preconceived opinions. As long ago as 1851, I urged that the method of observation and investigation then used, and which has been persisted in until now, is inadequate and erroneous, and therefore prevents the attainment of that success which in other sciences has so amply rewarded the investigator. At that time, I exhausted all my means in the endeavor to organize in the United States a corps of volunteer observers, to be connected by the telegraph, just then come into use, but my exertions failed. While, however, such organizations are now in full and systematic activity over almost the whole globe, the same method of research remains in use, and, until it is superceded, I cannot but fear that the desired results will be unattained.

The preconception we may have of the manner in which phenomena occur, necessarily exerts a great influence on what we see and how we see it. He who travels on one road, will receive one set of impressions, and he who takes another road, another set, and neither may adequately represent the characteristics of the country at large. So, in Meteorology, if we stick to the beaten paths, we may find ourselves but travelling the by-ways in which our predecessors have fruitlessly wandered. If we start with the most widely accepted meteorological theory,—that storms are essentially of but one kind,—*cyclones;* that they consist in an area of low barometer only; that they are practically, except in degree, the same throughout their existence and have, to use a metaphor, no embryonic changes and no development of nature:—then the old method of taking observations at fixed and arbitrary hours may be justifiable and even satisfactory; and the use of the method of averages to deduce laws from the mass of facts thus obtained cannot be gainsaid, for we are proceeding on the assumption that we are dealing with phenomena all of the same class.

But if, by adopting another method of observation, we are forced to the conviction that storms are not all of one nature, but that there are characteristic types, differing in origin, movement and appearance, that they develop and change continuously during their existence; that they do not consist of merely an area of low barometer, but that the areas of high barometer in the front and in the rear belong equally to the storm; that areas of high barometer, instead of being disconnected, independent and opposite phenomena, to be investigated by themselves, are in intimate connections with the areas of low barometer:—if we believe these things, then it is evident that the method of treatment, which throws all our data into one heap and averages them, as if all related to but one class, is insufficient, uncertain and utterly untrustworthy; because, only under exceptional circumstances, can it lead to positive results.

It is almost as if the chemist were to throw his materials into one pot, and sagely announce their average color, or their average taste,—or the zoologist to drive his animals into one pen, and gratify his scientific instincts with their average weight. Yet the meteorologist expects us to value his results, when he expends patient labor in finding the "average direction of storms," or the "average force of the wind," or the "average amount of rain-fall." How much more useful can these be than the average motion of the planets? No science was ever built up in this way.

I have gone over this ground pretty thoroughly in my work,* published about two years ago, by exposing the defective method of my own early investigations, many years since, and by an examination of the results of the labor of others. Among these latter, was the first paper† of Prof. Loomis's series of studies of the U. S. Signal Service weather maps, which came into my hands shortly before my work went to press. Seven of these papers having now appeared in the successive January and July numbers of the same journal, I refer to them again in this connection, because they illustrate very clearly the deficiencies of method by which such studies are in general pursued.

Prof. Loomis begins his study of the Signal Service maps, in his first

* Storms, their Nature, Classification and Laws. Philadelphia: Porter & Coates. 1875.
† American Journal of Science and Arts, July, 1874.

paper, by research as to the direction and velocity of storms, and "selecting" a certain number of "suitable cases" from records for two years, he subjects them to the method of average; but, as he notices great diversity in individual cases, he rightly concludes that the average thus reached is not a very valuable generalization. He, therefore, turns his attention to the more important question of the origin and cause of movement of storms. From the material at his hand, and by "suitable selection," he finds that the rain-area lies extended in the direction of the storm's progress,—in front of the area of low pressure—and he holds the opinion that rain is a potent agent in the origin and movement of areas of low barometer, which alone he recognizes as storms. Thus he says,* "a slight fall of the barometer was observed in Montana, probably the result of a fall of rain on some of the mountains of that region." In quoting his language, on page 173 of my book, to show his views as to the agency of precipitation, I have said :—"now, even if it were true that the rain-fall is the origin of motion in a storm, and its chief guiding power after its movement has begun, we should not have advanced, because the question as to the causes producing the rain-area would be fully as imperative as the other." On the appearance of this work, Prof. Loomis wrote to me :—"on page 173, you say that 'storms frequently travel over a considerable distance, unaccompanied by any rain-fall at all, and in most cases, the motion of the storm has already begun before the rain begins to fall.' Will you please give me the dates of some cases upon which you rely to establish that statement?" I replied that such cases could be found in abundance in the Signal Service maps, called his attention to a particular case, and reminded him of the rainless storms of the desert. At that time, therefore, he certainly did not believe in the statements he quoted, in his letter to me and did believe that rain-fall is a prime factor in the origin and cause of the motion of storms, being by his study of the maps.

In succeeding papers, however, he finds that, by his method of research, the same material as before just as readily confirms opposite views. It is only after two years, however, that he finds what he might have found at first, had he looked for it. In his seventh paper† he

* American Journal, July, 1874, p. 14.
† American Journal, July, 1877. pp. 13 to 18.

notes a number of "areas of low pressure without rain." He mentions one,* of which he says : "during these forty-eight hours not a drop of rain was recorded at any station within an area of pressure less than thirty inches, although on the 20th of October, this area had a diameter of 1500 miles," and he finds that, from the large majority of Signal Stations within this area, the reports showed either no clouds, or the sky only partly cloudy. He says :† "There seems to be no room for doubt that the barometric minima sometimes form with little or no rain, and continue without any considerable rain for eight hours, and sometimes for twenty-four hours and longer. These barometric minima seldom continue stationary for eight hours, but almost invariably travel to the eastward." Farther on ‡ is the conclusion, and it is italicized : "*that rainfall is not essential to the formation of areas of low barometer, and is not the principal cause of their formation, or of their progressive motion.*"

Now here is a view completely opposed to the first one, and yet both are based on the same data. His method of research had caused him to "select" those cases that squared with his pre-conceived opinion, and he did not think to look for storms without rain, until his attention was called to them. Surely, there is something radically wrong in the method that allows of opposite results from the same material.

And again—one of the arguments presented in my book, against Professor Loomis's theory of the rain-motor, was that there are storms having the rain area in the *rear*. Now Professor Loomis, in his first papers, does not find any such cases, but, after his attention is called to them, he has no difficulty in finding them. For instance, in his seventh paper,§ he says : "it is remarkable, that the centre of low pressure moved towards the north-east, having the centre of principal rain-fall almost exactly in its rear ;" and "these two cases, together with No. 12, on page 15 of my last paper, indicate that in the neighborhood of Kentucky, (*sic*) it is not uncommon for the principal rain-fall to occur after the centre of low pressure has passed eastward."

* Ibid. p. 17.
† Ibid. p. 16.
‡ Ibid. p. 18.
§ Ibid. p. 7.

Thus it is evident that his method of procedure caused him to overlook—until his attention was directly called to the matter—phenomena that are entirely subversive of the views he at first held, and that were just as patent first as last. Yet he is probably the best known of American meteorologists, and but worked in the regular way.

In his second paper, which * also was published before my book came out, Professor Loomis goes on to investigate the character and nature of the areas of high barometer, or "*anti-cyclones*," phenomena that appear to have greatly puzzled investigators. His proceedings are as follows: "I selected all those cases in which a maximum pressure, or high barometer, was so situated that the direction or velocity of the wind were given at a considerable number of stations for at least half of the entire area. Then, placing a wire cross upon one of the weather maps, over the centre of an area of high barometer, with the wire pointing north-east and south-west, the area was divided into four quadrants, which were designated as the north, east, south and west quadrants. Then, beginning with the west quadrant, I counted the number of stations at which the wind was reported from the north, also the number of stations at which the wind was reported from the north-east, the east, the south-east, etc., and in like manner for each of the four quadrants. The velocity of the wind, for the stations of observations in the different quadrants, was also noted. The same was done with each of the weather-maps which furnished an example suited to this comparison. The total number of cases derived from the weather-maps of two years (1872–73) was 188. All observations near the points of maximum pressure were rejected, generally all stations included with the first Isobars. Also no observations were employed beyond the Isobar 30.00, and generally none beyond the Isobar 30.10. I then found, by addition, the aggregate number of observations for each direction of the wind in the several quadrants, and from these numbers computed the wind's average direction for each quadrant. The average velocity of the wind, for each quadrant was also determined."

After this careful "selection" and preparation of the phenomena for the occasion, he subjects them to average and gets, naturally, the

* American Journal. January, 1875.

commonly accepted qualities of the "*anti-cyclone*,"—that the wind rotates in a direction opposite to that in a cyclone, and that it is accompanied by clear weather,—in short, in every respect the reverse of a cyclone. In all cases, in these two earlier papers, the area of high barometer is treated as a phenomenon entirely independent of the area of low barometer, except as "exerting an important influence upon it." He speaks only of *one* area of high barometer and *one* area of low barometer in connection, seeks their relative position and movements, and finds that the latter moves toward the former. It is evident, however, that Professor Loomis's views have undergone a change since my work appeared. I set forth the conviction that the progressive storms of the temperate zone consist in an oscillation between the tropical and arctic belts of high pressure (or rather parts of them), or in other words, in a system of opposing air-currents of different temperature. To avoid misunderstandings, I translated (p. 163) my terminology into that which generally is used, and say: "Regions of high barometer are identical with the regions of the polar and the equatorial currents, and the regions of low barometer, or 'centres of depressions,' the regions of calm between them." According to this, the storm consists not only of the area of low barometer, but of *two* areas of high barometer, which, so to speak, create the area of low barometer between them, by the upward flow of the equatorial current in front of the polar current, and all three move in conjunction. And, after two years of laborious study, we find Professor Loomis coming to something of the same sort. It should be remembered that, in his earlier papers, he had never spoken of "areas of high barometer on opposite sides of the low area." In his seventh paper he says: "The barometric minimum, October 19th, appears to have resulted from an area of high barometer (30.35), in the neighborhood of the Ohio valley, combined with an area of high barometer in Oregon. This excess of barometric pressure on opposite sides, caused a general movement of the intermediate atmosphere towards the valley of the upper Missouri, and each of these currents being deflected to the right by the rotation of the earth, a minimum pressure was caused over the region between the Rocky Mountains and Lake Superior. These two areas of high barometer, on opposite sides of the

low area, were remarkably persistent from October 19th to 21st, but advanced eastward at about the same rate as the barometric minimum."*

And so, also, in regard to the generally accepted notion, that these areas of high barometer—"anti-cyclones"—are unaccompanied by rain. I have shown, in opposition to this, that the most destructive storms and heaviest rains are caused by the advance movements of these areas of high pressure; as, for instance, the Nova Scotia storm of August, 1873, where the area of high barometer passed for two days through the jurisdiction of the United States Signal Service, without being recognized as a storm until, on its arrival at the coast, it destroyed more than a thousand vessels and six hundred lives, in almost a single night.

In his first papers, Professor Loomis does not find any instances of rain-fall produced under the influence of areas of high barometer; he holds consistently to the ordinary view, and finds warrant for doing so in his researches. But in his sixth paper,† we find: "We thus see that great rain-falls may occur under the influence of an area of high pressure as well as of low pressure." And—"from the preceding statement, we perceive that, in the Uuited States, south of latitude 36°, great rain-falls are accompanied by a cyclonic movement of the air, which sometimes appears to be the result of a neighboring area of low pressure, and sometimes of an area of high pressure, and that the latter case is about as frequent as the former. . . . . Extensive rain-areas sometimes occur in the Northern States, at a great distance from a low centre, where they appear to be as much under the influence of a centre of high pressure as of low pressure." ‡

Professor Loomis's change of view, in regard to sudden and great fluctuations of temperature, is also of significance. Already in his second paper, § he is struck with the occasional occurrence of re-

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* These three areas of high and low barometer are, in my view, parts of the storm, and *must*, therefore, move in conjunction, and with the same general velocity.

† American Journal, January, 1887.

‡ Professor Loomis would, therefore, agree with me in thinking that the rules for navigators, based on the theory that the storm is comprised in an area of low barometer, are worse than useless. And, in this connection, it may be stated that Commodore Wyman, the able Chief of the Hydrographic Office of the United States Navy, after a lengthy test, has recently recommended my book for use in the Navy, saying: "It is borne out by my experience." He admits that the old nautical rules for manœuvring vessels in storms are of little value.

§ American Journal, January, 1875.

markable changes in temperature, and comes to the conclusion that the cold changes are caused by sudden descents of cold air from the upper regions of the atmosphere. He does not believe they are due to a lateral movement of cold air from the north, and says definitely :— "If our observations covered the whole area of North America, I have little doubt we should find that the depression of the thermometer below its mean height, was greater in the United States than it was in the region north of us." He mentions changes of 5° to 10° taking place in a few minutes, and says :—"These sudden gusts of cold air are believed to descend from the upper regions of the atmosphere." We read :* "On the 14th of January, 1875, the thermometer at Denver had been below zero all day, with a variable north-east wind. At 9 P. M. of that day, the thermometer was one degree above zero. The wind then veered suddenly to south-west; at 9.15, P. M., the thermometer stood at 20°; at 9.20, P. M., it stood at 27°; at 9.30, P. M., 36°; and at 9.35, P. M., at 40°; after which, there was but little change till near noon of the next day. The preceding observations show a rise of the thermometer, amounting to 39 degrees in 35 minutes.

"On the 15th of January the thermometer had been above 40° all the morning, with a fresh south-west wind. About 11.30, A. M., the thermometer stood at 52°. The wind then suddenly backed to north-east, and at 12.30, P. M., the thermometer stood at 4°; being a change of 48° in one hour. Another observer, who is pronounced perfectly reliable, says that, between 11 A. M. and noon, a thermometer fell from 58° to 22° (that is thirty-six degrees), in five minutes. . . . . On the 14th, the barometer fell from 24.83 to 24.40 inches, and on the 15th, it rose again to 24.76 inches."

Professor Loomis thinks : "These changes of temperature and pressure which were noticed at Denver, were the effects of a considerable storm which came from the north-west." He goes on to say : "I do not think that these sudden changes can be fully explained by the supposition of a polar current sweeping along the earth's surface from a higher to a lower latitude, but it seems necessary to admit a sudden

* American Journal, July, 1875. p. 12.

transfer of very cold air, from a higher to a lower level. The heat of January 14th, probably resulted from a sudden precipitation of vapor, caused by the elevation of air from the earth's surface, and this warm air near the earth's surface suddenly ascended on the 15th, being displaced by colder air of a greater elevation."

Now, in the *Storms: their Nature, Classification and Laws*, I clearly ascribed all such sudden and violent changes in the northern part of the temperate zone, to a lateral movement of the air, which the change in the wind that invariably accompanies them, proves clearly enough. I assigned their cause solely to the advance of large bodies, or "areas," of warm or cold air, respectively from a southerly or a northerly direction. On page 79 is this: "With these changes of wind, there must necessarily be experienced a change in temperature. There is sometimes found in less than fifteen minutes, a difference of from 15° to 30° Fahrenheit."

In his fifth paper, Professor Loomis adopts the same explanation, and referring back to his former paper, quoted above, says: "These results appear to explain the facts mentioned in my third paper showing that a great diurnal change of temperature is most common at stations near the eastern slope of the Rocky Mountains. The cold wave (*sic*) makes its first appearance in this region, and the intensity of the cold is sensibly diminished as the wave travels eastward. An example of the variable climate of the eastern slope of the Rocky Mountains, occurred December 24th, 1872. Denver was at that time *on the border of the cold wave*,* which prevailed from the Rocky Mountains to Nova Scotia, and during the night of the 23d and 24th, the thermometer fell to 2°. During the 24th, Denver began to feel the influence of the storm which was advancing from Oregon, and on that day the thermometer rose to 55°, showing a change of 53° in a day, and probably the entire change took place in less than 24 hours. Similar cases must frequently occur near the eastern slope of the Rocky Mountains, and the changes of temperature are more sudden there than they are near the Atlantic coast, because the cold which succeeds a storm, is more intense than it is in the eastern portions of

*The italics are mine.

the United States." There is no longer any mention of the sudden descent of large bodies of cold air; that is abandoned, and lateral changes only are recognized.

In his seventh paper,* Professor Loomis finally arrives at some idea of storms that consist of a system of two opposing currents of different temperatures, which for days keep in the same position, and therefore do not rotate. His positions are in direct opposition to those of his earlier papers, and they bear a striking resemblance, in many points, to the principles I have set forth in *Storms: their Nature, Classification and Laws*, which had appeared in the meantime.†

The following excerpts from this seventh paper, show a very different basis of opinion from the earlier papers, so complete a change, in fact, as to make some explanation necessary, one would think:

"On the morning of October 19th, 1873, along the coast of North Carolina, Virginia and New Jersey, light winds from the east or south-east generally prevailed, while west of Virginia and Pennsylvania, the winds were generally from the west and north-west. This opposition of winds was attended by rain-fall, which in the afternoon became general along the Atlantic coast, from Wilmington to Boston, and extended inland 300 or 400 miles."

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"This cooler wind from the north-west, probably flowed under the south-east wind from Memphis, causing it suddenly to ascend, and thus produced a rapid precipitation of vapor."

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"Northerly winds commenced blowing from the neighborhood of Lakes Superior and Huron, being the result of a lower temperature and a higher pressure. This colder wind from the north probably flowed under the south-east wind which had been blowing over Lake Michigan, and produced that strong upward movement of the air

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* American Journal, July, 1877.

† Professor Loomis's earlier views as to the origin of storms, rain and snow, can also be seen on p. 15 of his first paper (*American Journal, July*, 1874), where he says: "It seems probable that this storm originated, or at least was first developed into a storm of considerable magnitude, through the collision of moist air from the Pacific Ocean with some of the high mountain peaks in Oregon, resulting in a heavy fall of rain or snow."

which was followed by excessive rain at Grand Haven and Milwaukee."

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"On the evening of November 6th, the wind from the south and east generally prevailed along the entire Atlantic coast, and these being opposed by westerly winds (the result of high barometer in Tennessee), there was an exeessive rain on the night of November 6th, which was especially heavy along the coast, from Georgia to Massachusetts."

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"No. 22 appears to have resulted from east winds along the Atlantic coast, opposed by west winds near the Mississippi valley, on the south side of an area of low pressure."

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"In No. 19, the winds upon the Atlantic coast, near Philadelphia, were generally from the south or south-east, while at a distance of three hundred miles from the coast, the winds were from the west or north-west. It seems probable that this north-west current crowded under the south-east current, lifting it up from the earth's surface and thus condensing its vapor."

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"In No. 47, as has been already mentioned on p. 4, the centre of the rain-area was on the north-west side of the centre of low pressure. It seems probable that in this case the violent south-east wind from the ocean extended further west than Buffalo, and that its vapor was condensed by its being elevated from the earth's surface by the crowding of the north-west wind beneath it."

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"In No. 40, south winds generally prevailed in Georgia and the Carolinas, with the cold winds from the west and north-west in the north-western States. This westerly current probably pushed under the south wind from the ocean, and, lifting it up from the earth's sur-

face, condensed its vapor, and it is presumed that the south wind prevailed as an upper current, at many places where the north-west wind prevailed at the surface. . . . . This upper current from the east is generally concealed by the lower clouds, whose course is generally the same as that of the surface wind; but when the lower clouds are broken, the movement of this upper current can sometimes be seen." *

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Now, in all this, there is an introduction of principles entirely foreign to anything Prof. Loomis had before written. We see a constant recognition of the opposition of two air currents, of different direction and temperature, in a storm; of the cooler current flowing under and lifting up the warmer one, and thus producing the rain; and, indeed, of movements of air being the result of differences of temperature and therefore of pressure, and of other vital principles that are first set forth in *Storms: their Nature, Classification and Laws.* And in his recent essay, read at the late session of the National Academy of Sciences, but not yet published, he seems to have still further committed himself, to judge from the synopses that have appeared in the prints. The explanation he gives of our great north-east storms is, in general, precisely like my own, varying only in details, and except that he curiously mixes up with it a remnant of his old rotary views. He has adopted positively the principle, already laid down by myself, that "rain increases the force of a storm, though

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* All this, it will be seen, bears a striking resemblance to the following, from pages 48 to 50 of *Storms; their Nature, Classification and Laws.* "Horizontal currents of different temperatures, moving in opposing directions, overlap each other. The warmer, rising obliquely over the cooler current, moves to the cooler region, while the cooler current flows over the surface of the earth, beneath the warmer current, to the warmer region. . . . . Air may be cooled from below as well as from above. . . . . When a warm current of air moves obliquely up over a cool current, this sort of cloud-formation from below, often takes place in the region where the two currents meet and mingle; and it happens sometimes that the veil of mist thus formed above us, is sufficient to shut out from view the upper clouds. In the movement of a warm current to cooler regions, it may happen, when the stratus is thus concealed by the mist-cloud of the plane of meeting, that the cool current becomes sufficiently heated to dissolve the mist from below, and the stratified appearance of the upper clouds will then suddenly become visible."

". . . . . where the cool current moves horizontally, it shifts over the ground like a wedge, with its lower edge foremost. The warm air is thus lifted or forced up, and flows over the cool current, as in the other case, ... and its moisture will condense," and, if abundant enough, precipitate.

never originating it," which, he says: "may also be deduced from the fact the inflow begins before there is any precipitation of rain." This is what he disbelieved when he wrote to me, asking for cases, several years ago.

And so, also, the principle that the storm does not consist in the area of low pressure *only*, but in *two* areas of high pressure which create the area of low pressure between them and travel in conjunction, and that the rain area is as much under the influence of high as of low pressure.

These two principles were never set forth until I published the *Storms: their Nature, Classification and Laws*, in 1875, and, in adopting them, Prof. Loomis has committed himself to the system I hold, in opposition to the views he at first found confirmed in his researches. The fact that he applies my principles, without mentioning the source from which he obtained them, may be taken as evidence of the confidence he places in their truth. But what can we say of the system of research that first furnishes him with results only to be thrown aside, without as much as an explanation of their fallaciousness, when contrary views are brought to his attention? And yet he, like others, was working in the regulation method, in which the great bulk of meteorological investigation is being carried on the world over. And this method, with all his careful labor, but yielded him its legitimate fruit; it will always seem to confirm the preconceptions with which one starts out, and thus lead only to a number of unconnected, vague, contradictory and confusing results by various investigators. Truly, we cannot wonder when men like Airy say, "that meteorology is no science," and, in reading such results, we sympathize with the pupil of Mephistopheles, in *Faust*, when he says :—

> "Mir wird von alle dem so dumm,
> Als ging mir ein Mühlrad im Kopfe herum."

And we cannot fail to admit, I think, that, to a defective and inadequate method of observation and research, is chiefly due the slowness of the progress that meteorology has made and is now making.

Biot, after enumerating the efforts to advance this science, says :

'What will come of it? Nothing, and nothing will ever come of it. No single branch of science has ever been fruitfully explored in this way."

And Sir William Herschel says: "In endeavoring to interpret the weather, we are in the position of a man who hears, at intervals, a few fragments of a long history related in a prosy, unmethodical manner. A host of circumstances omitted or forgotten, and the want of connection between the parts, prevent the hearer from obtaining possession of the entire story."

And Sir G. Airy: "Whether the effect of this movement will be that millions of useless observations will be added to the millions that already exist, or whether something may be expected to result which will lead to a meteorological theory, I cannot hazard a conjecture."

And Proctor: "At vast expense, millions of records of heat, rainfall, winds, clouds, barometric pressure, and so on, have been secured, but hitherto no law, at least, from which any constant system of prediction for long periods in advance can be deduced."

Now why is it that "millions of observations" are heaped up "at vast expense," only to be useless? The answer is not difficult. It is, as Sir William Herschel says, "because of the host of circumstances omitted or forgotten, and the want of connection between the parts." This covers almost the whole cause of failure.

Another disappointment, I fear, will be the attempt to make meteorology an exact science, and to persuade the winds to move in mathematical figures, and according to mathematical formulæ. The origin of this endeavor, is, without doubt, to be found in the connection of astronomers with the beginnings of meteorological science, and the influence astronomy has always exerted upon it. Who supposes that we shall ever be able to calculate a storm as we do an eclipse? And until investigators rid themselves of the notion that the winds move as the planets do, or that their force and motion can be expressed in a mathematical formula, just so long will they lack a true conception of the elastic and variable movements actually to be found. And, on the other hand, so long as they are content with arbitrary

and detached observations, just so long will they befog themselves with averages and other hodge-podge results. Each science has to find for itself the mode of procedure that is suitable to the nature of its subject, and, until that is found, its development will be slow.

In conclusion, I have to thank Prof. Loomis, for adopting, although in rather a disjointed fashion, some of my views; the compliment he pays me is the greater, in that he fails to acknowledge his indebtedness.

WILLIAM BLASIUS.